the new point of view with a mind as free as possible from prejudice, and with a single eye to the truth. In short, the ideal investigator is the scientific independent, the chemical 'mugwump.' It is too unreasonable to hope that the problems of the twentieth century will be dealt with in this thoughtful but untrammeled fashion?

We Americans rejoice in having on our side of the ocean the world-renowned names of several great men, of Wolcott and Willard Gibbs, of James Crafts, Edward Morley, the late Josiah Parsons Cooke and others, who have combined chemistry with physics and mathematics; but, nevertheless, one must admit that America has not done as much as one could wish toward building up the fabric of modern physical chemistry. Although science is world-wide, and scientific men should be cosmopolitan, the existence of this Association proves that there is a patriotic side to the matter too. While welcoming the truth, wherever it is discovered, let us then do all we can to further its emanation from American laboratories and writing desks.

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*A CENTURY OF PERSONAL EQUATIONS.**

IN 1795 Maskelyne, Astronomer Royal at Greenwich, discovered that his assistant, Kinnebrook, was in the habit of noting star transits about seven-tenths of a second of time later than himself, and discharged the poor fellow as 'vitious' in his method of observing. The matter attracted little attention until, about twenty-five years later, the celebrated Bessel investigated it, and showed that the best observers whom he could influence exhibited similar discrepancies in their transits. Bessel himself was exceptionally early in his times, and found that other astronomers were usually later. The theory which he formed was that the early observers, Maskelyne and Bessel himself, heard their clock beats before they saw the stars' images, while the late observers, Kinnebrook, Argelander, W. Struve and others, saw first and then heard. The theory of Bessel has been generally adopted by astronomers and psychologists, and the investigation of the differences between astronomers has been pursued pretty continuously since 1836, when Airy, as Astronomer Royal at Greenwich, began a regular continuance of Bessel's investigation soon after entering upon that office. The matter was more or less perplexing to the Greenwich observers for the twenty years between 1836 and 1855. In 1853 the so-called eye-and-ear method, which had been employed for about a century previously, was laid aside at Greenwich for most purposes, and replaced by the American, or chronographic, method of galvanic registration, invented by Sears Cook Walker in 1849.

During the first half of the century, 1795 to 1895, to which this paper refers, observations of transits were made by Bradley's method, or by eye and ear, but for the second half century observers have had the benefit of Walker's invention, and of the ingenious apparatus constructed by the Bonds and other mechanicians for the purpose of carrying out the principle introduced by Walker. The investigations of personal equation up to 1853 are based, then, upon experimental psychology as developed by Bessel, and have led to a pretty complete body of empirical facts in that direction. But Bessel and his associates considered the whole matter enigmatical and difficult to trace, owing to the fact that the phenomena are subconscious and not easy to bring under the laws of experimental science. Observers noted large differences in their times, a second or more, and could not reduce them to moderate

* See also my article in SCIENCE for Nov. 26, 1897.

amounts by long practice. A second of time in longitude amounts to fifteen hundred feet on the map if the place be near the equator, so that, all told, the elimination of personal equation is one of the most important and perplexing problems of practical astronomy.

The matter became more easily handled on the introduction of the chronograph in 1849, for several reasons. In the first place, the Greenwich observers found that by the new method the personal equations were diminished in amount in a general way. Sir George Airy, in his report for 1854, sums the matter up in these words:

"This apparatus, the chronographic, is troublesome in use, consuming much time in the galvanic preparation, the preparation of the paper, and the translation of the puncture indications into figures." And in his report for 1855 he also says: "The magnitude of the personal equation in the galvanic-touch method is not above half of that in the eye-and-ear method." But among the observers who use it there is but one opinion on its astronomical merits, that in freedom from personal equation and in general accuracy it is very far superior to observations by eye-and-ear method. This judgment of Airy's, however, needs some slight modification, according to the opinions of many of the best practical astronomers now living, and it is worth while to look at the other side in order to see if the eye-and-ear method should be kept up in active practice. First of all, as a method of training young observers it has some importance, as the apparatus is greatly simplified if the galvanic connections and preparation are eliminated. It is also often necessary to make time observations at so great a distance from civilization that the delicate chronograph is better left behind. This is a practical difficulty I have often experienced in geographical work in New Mexico and other distant portions of the United States; no chronograph was furnished me, and it was possible to fix the position of a corner post of Wyoming without a chronograph with an accuracy quite unusual in the U. S. Land Office at that time. Similar considerations were of importance in the geographical mapping work of the U. S. engineers, where the so-called station error or irregularities in the surface of the geoid far exceeded any errors arising from the use of the eye-and-ear method. After the great Chicago conflagration of 1871 it was a piece of good fortune that I could use the eye-and-ear method, as I was engaged in geographical operations for the U. S. engineers, who did not then possess a sufficiently complete supply of chronographs. In 1868 began the observations of the great international star catalogue of the Astronomische Gesellschaft, which is now approaching completion after thirty years of steady observation. At that time the Council of the Society were undecided as to the use of the chronograph in their catalogue, and its use or non-use was left to the discretion of the observers. In my own case I decided to begin without one, as the Chicago Observatory, where I then was, had not provided money for it, and the chronograph now used by my friend, Professor Hough, at his new observatory at Evanston, was constructed later. The conflagration, in its consequences, put an end to my work upon a zone of the A. G. C., and the zone continued at Lund, Sweden, by an appropriation from the Swedish government, and is, I suppose, nearly completed; but I went far enough, by the eye-and-ear method, to satisfy myself that it would have been entirely practicable to go on and satisfy the requirements of the Council as regards accuracy. At Harvard College Observatory my lamented friend, Professor W. A. Rogers, used an excellent Bond chrono-

graph, and completed his zone about a dozen years ago. Other observers decided for themselves whether or not to employ the chronograph, with the general result that with it the zone would be rather more accurate on the surface, and without it would be rather more promptly completed. When I say rather more accurate on the surface I mean that chronographic registration appears to be especially liable to a peculiar form of personal equation, viz.: a variation of the time of transit and, consequently, of the resulting right ascension, when the star is fainter than the ordinary stars observed for clock correction. This matter was pointed out originally as essential to be investigated, but has not yet been fully cleared up. So far as chronographic observations are concerned, there seems to be no doubt that the effect of faintness upon the time of transit is to delay the reaction or registration very generally, if not absolutely without exception. But, on the other hand, there are several observers, Argelander, Bauschinger, Deichmüller, Copeland and Börgen, for whom stars near, but below the limit of easy observation, with the instrument employed, are observed by Bradley's method earlier than brighter stars, while the same observers note the transit of stars near this limit, but above it, quite normally. This feature of his own observations was detected by Argelander himself, and confirmed by Auwers in his careful discussion of his own Berlin zone, in which the Bonn observations are taken into account.

As the phenomenon detected by Argelander in his own observations was referred to a psychical cause, it is likely that other observers might become aware of a similar phenomenon in their own observations, if it were not that the differences are trifling and liable to mislead the investigator who shall attempt to reproduce them, as is sufficiently apparent when the attempt is made to introduce a strict logical order into the statements already published.

Personal equation is a subject so different in its causes from the ordinary instrumental peculiarities which manifest themselves in results that the causes of it, which are psychical, are entirely liable to be mistaken and thus obscured, and entirely trustworthy results are liable to be rejected as abnormal, because they do not agree with groundless hypotheses. Suspicion has been expressed, for example, that Nyrén's latitude observations, with the prime vertical transit of the Pulkova Observatory, are liable to an equation of a personal nature depending upon the magnitude of the star observed. The suspicion was based upon the theory that the chronographic and eye-and-ear methods have some elements in common, which rendered them equally liable to such a form of personal equation, while the fact is that the general phenomena of personal equation by eye and ear are due to the cause detected by Bessel, viz.: the 'Zeitverschiebung,' or displacement of time, which arises when the attempt is made to add an impression on the sense of hearing to one at exactly the same instant on the sense of sight. In the chronographic method of registration the time required is in normal instances positive—that is, the 'reaction' time of the psychologists. The two methods of observing transits are psychically different, and the general result for ordinary time stars is that the average chronographic observer produces transits about as much later than the average eye-and-ear observer as is required for a simple reaction. The amount is 0.162 at Greenwich for the ten years 1885 to 1890, inclusive, and 1890 to 1894, inclusive, with trifling fluctuations (see my paper in No. 425 of the *Astronomical Journal*). Since writing that I have received the introduction to the Greenwich Astronomical Observations for the year 1895, which gives a result almost identical with the years from

1885 on; for 1895 we find $e' - e = 0^s.161$ for 13 observers in all. The difference $-0^s.001$ between the mean for ten years and that for the single year 1895 is far less than the probable error about $\pm 0^s.002$ of the mean for 1895, a decided indication that the quantity $0^s.16$ is obtained with substantial accuracy from the ten years' results, and represents something which arises from a true cause or combination of true causes. A persistent positive sign of the quantities $e' - e$ is due, as it seems, to the fact that the chronographic transits are registered too late, combined with the other fact that the eye-and-ear observations are for some observers too late and for other observers too early. In order, then, to obtain the true time of a series of transits, the chronographic method, if employed by all the Greenwich observers, would give an average time too late by about $0^s.16$, while the eye-and-ear method would give an average time $0^s.16$ earlier and more nearly correct. We may suppose, for example, that in 1895 the 13 observers whose eye-and-ear personal equations are discussed in the introduction for that year have observed each a star of the average magnitude of a Greenwich time star, and in a moderate declination near the average declination of time stars, and, reducing the observations in the usual way, have obtained a clock correction by each method, but without the application of the personal equation. The average of the thirteen chronographic clock corrections would then be $0^s.16$ too small, while that of the thirteen eye-and-ear clock corrections have no common error constant for the thirteen. The standard observer for 1895, Mr. Lewis, obtained by eye and ear a clock correction $0^s.10$ larger than by chronographic on three nights in that year, and hence, so far as these three nights show, his eye-and-ear transits are more nearly correct than his average chronographic clock corrections, as we cannot well infer that the actual reaction time occupied in the bisection is very far from $0^s.16$. For a series of ten years in all the two-method equation for Mr. Lewis has been $0^s.139$ in the mean or for separate years as follows:

$$
\begin{aligned}
1885 &+ 0.06 \\
1886 &+ 0.13 \\
1887 &+ 0.13 \\
1888 &+ 0.19 \\
1889 &+ 0.15 \\
1890 &+ 0.15 \\
1891 &+ 0.09 \\
1892 &+ 0.19 \\
1893 &+ 0.20 \\
1895 &+ 0.10
\end{aligned}
$$

No eye-and-ear observations were recorded for Mr. Lewis in 1894, and the largest difference from the mean, viz.: $-.079$ for 1885, is not as large as the corresponding difference $-.089$ for the chief assistant, Mr. (now Professor) Turner, for the same year. The probable error of a year's determination for Mr. Lewis is $\pm 0^s.033$ by the sum $0^s.37$ of the ten differences from $\pm 0^s.031$, and by sum of squares the mean error is ± 0.047 and the probable error $\pm 0^s.031$. The important question whether there is in general a variation of personal equation with magnitude has already been tested in a good many ways by various astronomers, with the general result that such variations are far more uniformly exhibited in chronographic transits than in those taken by eye and ear. The investigations of the effect of such a personal equation have been carried on for the following zones of the Catalog der Astronomischen Gesellschaft, already published:

	Place	Observer
Zone 1° to 5°	Albany	Boss
15° to 20°	Berlin	Auwers
20° to 25°	Berlin	Becker.

There are various other investigations for chronographic observers which all agree in general with the result of the reaction

experiments in psychological laboratories, viz. : that the time of reaction, like that of chronographic registration, is lengthened when the impression on the sense is faint. But for eye-and-ear transits the experiments with screens are so far few and somewhat indecisive, and the phenomenon detected by Argelander, viz.: an anticipation of the transit of a star faint enough to be a little difficult of observation, has been noticed by several observers and tested in various ways.

The suspicion is expressed in Number 369 of the *Astronomical Journal* that the variation of personal equations with the magnitude of the star observed affects equally eye-and-ear observations and those made with the chronograph.

But on reading over the article in question it is at once noticed that there is great lack of detail in the result quoted, and that the direct determination by Becker shows an anomaly which the author of the article in *Astronomical Journal* No. 369 is confessedly unable to account for. A careful reading of Becker's investigation in his Berlin zone shows that the observations were made with Professor Becker's well-known skill and care, and whatever difficulty there may be in reconciling these results with other observations is probably due to the treatment of the latter, and hence that the lacking details in *Astronomical Journal* No. 369 would, if supplied, perhaps account for the discrepancy.

The author of the article quoted has not, so far as it appears, used his conclusions in his later important investigations. Even his rather hasty decision in favor of such a variability of eye-and-ear personal equation with magnitude deserves careful study as to the facts involved.

I venture to suggest a line of observation which I desire to see carried out, and which will add to the certainty of these conclusions.

The Christiana zone 64° 50′ to 70° 10′ of the A. G. C. has been long completed by the late Professor Fearnley, by whom the transit observations were made by eye and ear, and his successor, Professor Geelmuyden, who made the observations for declination and most of the reductions. The observations for right ascension are liable to but small casual errors, and have, I believe, been shown to be nearly free from constant error due to the faintness of the stars below the magnitude at which they are easily observable. The stars of this zone below a certain magnitude, for which I may assume 8.2 of the B. D. scale, might be reobserved to some advantage in connection with a similar reobservation of Groombridge's stars, which is now going on at Greenwich. In order to conduct such reobservation to the best advantage, all things considered, I should confine it to those stars in Groombridge's catalogue which are within 25° of the pole for the epoch 1875, as the meridian circle in my charge has an aperture of four and one-half Paris inches, and there are very few of Groombridge's stars which it cannot easily reach, as I know by long experience with it. The few Groombridge stars, if there are any such, which would give any trouble with the Williams College circle to reobserve, are those which Groombridge picked up on exceptionally clear nights, but they are included in the Radcliffe Catalogue, whose right ascensions were observed with an aperture of considerably less diameter. The cases will be very few in which Groombridge's stars will not be easily observed on any good night with the aperture of 122 mm., and in which the observer would be liable to the 'Argelander phenomenon' or reversal of the ordinary order of sensations as shown in the cases of Argelander himself, Copeland, Börgen, Bauschinger and other good observers.

The difficulty of separating this form of

personal equation from other forms is very considerable. When the Greenwich catalogue for 1890 is published, it will be necessary to find out in some manner the personal error depending on magnitude of the chronographic right ascensions of that catalogue, but these will have in them a personal element depending on the habits of the observers by whom the transits have been registered, and this will be complicated unless it is shown that the various observers have been brought to a more uniform habit than is generally supposed. The comparison of the catalogue for 1890 with the zones of the A. G. C. will at a later time furnish a great amount of interesting information, but which at present needs the careful study of the methods of observation and elements of reduction which have been employed in the zones already observed by eye and ear as well as by chronograph.

The catalogue of Dr. Romberg* is the best standard of comparison for the A. G. C. eye-and-ear zones, as it was observed in the years 1874 to 1880 with a powerful meridian circle whose aperture was large enough to render all the A. G. C. stars distinctly visible, and the standard of reduction is the same as for the A. G. C., viz.: Wagner's right ascensions for 1865 and Nyrén's declinations for the same epoch.

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SOME DANGERS OF THE ABUSE OF CHEMICAL FORMULAS.

When Thomson made his memorable visit to Dalton, in Manchester, nearly one hundred years ago, and was shown the system of symbols by which Dalton hoped to make clear his ideas as to atoms and their combinations, he was enthusiastic as to the future usefulness of such a system. And, although the system was clumsy and inadequate to the task of properly representing the great mass of chemical facts, it contained the valuable idea of graphic representation which was to be ingeniously elaborated and developed by later masters of the science.

It was through Berzelius next that chemical symbols were made simpler and clearer. So manifest was their usefulness that they speedily claimed the additional advantage of almost universal acceptance. Local adoption only, the use by chemists of one nationality or the followers of one master would have proved a most serious bar to the advancement of the science. We can fancy the confusion which would arise from the use of different systems at present, but, happily, such a picture exists in the imagination only. The science has one universal language of symbols which those of every tongue can read and understand. The advantages of such a system need not be dwelt upon. I purpose rather pointing out a few possible dangers and abuses.

The simple application of symbols in the time of Berzelius has become more complicated as the science has developed and the knowledge of both composition and constitution of chemical bodies has increased. Intended at first to represent elements and single compounds, the symbols have been developed into complex formulas, and these have been united into algebraic equations in the effort to make them represent as much as possible of the knowledge so laboriously acquired by multiplied experiments. The system has become in truth the shorthand of chemistry.

While its great usefulness is not to be underestimated, the limitations of the system should be duly recognized. In the first place, it can only partially represent the mathematical relations of the science. Again, there is no mode of indicating in an equation the physical forces which always accompany chemical reactions. These re-

* I regret to say that this excellent observer has passed away since these words were written.